MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXIX. *April* 9, 1869. No. 6.

Admiral MANNERS, President, in the Chair.

The Rev. S. J. Perry, of Stonyhurst College,

was balloted for and duly elected a Fellow of the Society.

On the Rotation of Mars. By Richard A. Proctor, B.A.

Mr. Browning having mentioned to me several months ago, that he hoped to be able to obtain several drawings of *Mars* at the present opposition, by means of his 12-inch reflector, it occurred to me that I might avail myself of so favourable an opportunity to test the estimate I had formed of the rotation-period of *Mars*. It is true that, when an estimate is formed from the rotations occurring in the course of so long a period as 201 years, one cannot expect any important correction to be obtained by throwing in the rotations of another biennial period. But there was this to be considered:—The drawings of *Mars* which I had used in the former calculations had not been constructed with any reference to a computation of this sort. Those who are familiar with the difficulties inherent in the observation of *Mars* will recognise the fact that the draftsman might very well place a feature slightly on one side or the other of its true position. I have found this to be the case in drawings by the best observers, at least if the hour and minute appended is to be considered the true epoch at which the drawing is made. On the other hand, a simple observation having reference only to the central passage of some distinct marking is a matter about which there is much

less chance of mistake. The experiments of Sir Wm. Herschel as to the views which even unpractised observers form respecting the centering of a disk show how little chance of error there is in a matter of this sort; and in the case of a practised observer like Mr. Browning the chance of error was much diminished.

The special work, then, which I asked him to favour me by carrying out was the observation of a central passage of the Kaiser Sea (using the nomenclature of my chart of *Mars*) — that is, the long funnel-shaped sea running north and south, sometimes called the Hour-glass Sea. This feature is depicted in two views of *Mars* taken by Hooke on March 12th, 1666 (New Style), at $12^h\ 20^m$ and $12^h\ 30^m$; and my former estimates of *Mars*' rotation period had been obtained by comparing Hooke's views with drawings of the same feature by Herschel, Beer and Mädler, Dawes and Browning. From the close agreement between the two drawings taken by Hooke and the true figure given to the Kaiser Sea, I am disposed to place much more reliance on his observations than one would ordinarily accord to the work of the astronomers of the seventeenth century. There can be no doubt that he saw the Kaiser Sea under peculiarly favourable circumstances on that spring night in 1666, since his drawings will bear favourable comparison with any that have been obtained in recent times with instruments of moderate power.

So much for the observation which limits one end of the long period.

The observation next to be considered is the one which Mr. Browning obligingly made at my request. According to this observation the Kaiser Sea was centrally situated on the disk of *Mars* on February 4, at $11^h\ 3^m$ G. M. T. A few minutes before this hour Mr. Browning felt certain that the sea was to the left of the centre, and a few minutes after he felt certain it was to the right. This will be understood when we remember that a point near the middle of the disk traverses $\frac{1}{470}$th of the disk's diameter in a minute. So that if we take a range of eight minutes on either side of the epoch of central passage, we get a range of motion equivalent to about $\frac{1}{59}$th part of the disk's diameter, and there would be no mistaking so considerable a motion as this.

At the hour of observation the longitude of the Earth was about $135\frac{1}{4}$ degrees; that of *Mars* about $141\frac{1}{4}$ degrees. Marking down the points thus indicated upon my chart of the orbits of the four inner planets, and drawing in also the points occupied by the two planets at the epoch of Hooke's observation, I found the difference of *Mars*' geocentric longitude to be about $35\frac{1}{2}$ degrees. From the care taken in the construction I think this result may be trusted within a quarter of a degree at the outside. It results that the correction for geocentric longitude may be obtained by adding the period in which the planet rotates through an angle of $35\frac{1}{2}$ degrees, or by subtracting the period of rotation through $324\frac{1}{2}$ degrees.

Next, as to the correction for phase. It is obvious from Hooke's first drawing—see *Monthly Notices* for January 1868—that the Kaiser Sea was some 18 degrees (or perhaps rather more) from central passage at the hour of observation. Thus we must deduct a further interval equivalent to the period of the planet's rotation through somewhat more than 18 degrees, because Browning observed the planet when rotated so many degrees further than when seen by Hooke.

We have then, in all, a deduction equal to the period of the planet's rotation through $324\frac{1}{2}^\circ + 18\frac{1}{2}^\circ$ (say) $= 343^\circ$. This corresponds to a deduction of

$$\frac{343}{360} \times 88643 \text{ seconds,}$$

or

84457 seconds.

Now the total number of seconds between

March 12th, 1666, $12^h\ 20^m$,

and

February 4th, 1869, $11^h\ 3^m$,

will be found to be

6402926580 seconds,

and the corrected interval is therefore,

(6402926580 − 84457) seconds
= 6402842123 seconds.

The number of rotations obtained by dividing this period by a rough estimate of the rotation-period at 88642·7 seconds is found to be 72232.

Lastly, dividing 6402842123 by 72232, we obtain for the rotation-period the value

88642·736 seconds,

or

$22^h\ 37^m\ 22{\cdot}736$ seconds,

the value named in my last paper on the subject being

$22^h\ 37^m\ 22{\cdot}735$ seconds,

and the limits of probable error

0·005 seconds.

The three intervals before examined, ranging from 190 to 201 years, gave severally

88642·737 seconds
88642·734 „
88642·734 „

The interval now considered, one of nearly 203 years, gives a value between the greatest of these and the two others; and as all the values now obtained lie between

88642·73 s. and 88642·74 s.

we may assume that the planet's rotation period is fairly represented by the value

$22^h\ 37^m\ 22{\cdot}735$ seconds,

with a probable error of ·005 and a possible error of $\cdot 015^s$. More than this error, which gives a range of $\cdot 03^s$ for each rotation, and therefore $36^m\ 6^s{\cdot}96$ for the total number of rotations since the date of Hooke's observations, it would be unreasonable to admit, since the planet's change of appearance, even in a quarter of an hour, is perfectly cognisable by a practised observer.

Kaiser's period ($22^h\ 37^m\ 22^s{\cdot}62$) which differs from mine by $0^s{\cdot}115$, would throw the planet out, at the period of Hooke's observation, by an amount corresponding to the amount of rotation during $2^h\ 20^m$. This is not to be thought of for a moment. Hooke depicts the Kaiser Sea at a distance of rather more than 18° from the centre, and the change resulting from Kaiser's period would throw the Kaiser Sea nearly 50° from the centre. At this distance it would have been lost in the haze near the planet's limb, even had Hooke's telescope been as powerful as the best modern instruments. On the other hand, the fact that Kaiser was led to the period $22^h\ 37^m\ 22^s{\cdot}62$ by the comparison of some of the best modern drawings, taken over a widely extended period, suffices to prove that Hooke's drawing depicts the Kaiser Sea, and not the Dawes' Strait, which sometimes looks not unlike the Kaiser Sea, especially at that season of the Martial year in which Hooke observed the planet. For Kaiser's period throws the Dawes' Strait almost exactly in the centre of that hemisphere of *Mars* which would have been turned away from Hooke at the time of his observation.

The period deduced by Messrs. Beer and Mädler, $22^h\ 37^m\ 23^s{\cdot}8$, is disproved by Sir W. Herschel's drawings; otherwise it would fairly account for Hooke's drawing on the omission of a complete rotation. Herschel's views coming midway between Hooke's and modern drawings satisfactorily remove any doubt of this sort.

On the Meteoric Shower of November, 1868, *as seen at the Royal Observatory, Cape of Good Hope.* By Geo. W. H. Maclear, Esq.

I have the honour to forward for the information of the Royal Astronomical Society, the result of the watch kept up at this Observatory, on the 13th, for the November meteoric shower.

The first meteor from *Leo* was noted at $13^h\ 18^m\ 30^s$, Cape mean time.

Shower at its maximum about $14^h\ 42^m$.

Last meteor noted (in broad daylight) at $16^h\ 27^m\ 20^s$.

The total number observed amounted to 134, but a dense haze prevailing over the eastern horizon doubtless obscured many others from view. For the same reason we were unable to observe the exact radiant point. Tracing back some of the brighter ones, it appeared to be between γ and ζ *Leonis*, and a little to the north of γ.

No meteors were noted on the 12th of November.

On the 14th the sky was clouded.

Meteoric Shower of November 13th, 1868.

Cape Mean Time. h m s	Direction. From	To	Remarks.
13 18 30	Leo	Zenith	White, large
27 40	,,	Westward	Orange and greenish, large
34 35	Hydra	E. horizon	Small
36 32	γ Leonis	S.	Faint
49 0	Leo	Zenith	White and green
52 5	,,	S. along horizon	,,
57 50	α Hydræ	Horizon	Small
57 50	,,	,,	,,
13 58 5	γ Leonis	Horizontal to S.	,,
14 1 45	π Leonis	Zenith	
3 5	,,	S.	
6 10	,,	S.W.	
7 45	,,	,,	
11 30	Leo	S.	
12 10	Below Regulus	Horizon	Blue and green
14 30	Regulus	S.	Faint
15 20	,,	,,	,,
15 50	,,	,,	,,
17 20	Above Regulus	Horizon	,,
18 35	Leo	S.W.	Orange, large
20 27	,,	Horizontal to S.	
22 20	,,	,,	Small, no train
22 32	,,	,,	Large
23 10	,,	Zenith	,,

Cape Mean Time. h m s	Direction. From	To	Remarks.
14 23 33	Leo	S.	
24 47	Below Leo	Horizontal to S.	Small
25 0	,,	,,	
25 40	,,	,,	
26 50	,,	,,	
28 10	,,	,,	
30 58	,,	,,	
32 45	,,	,,	
33 55	,,	,,	
34 15	Below ♂	Horizon	
35 30	Regulus	S.W.	Orange, large
36 5	μ Leonis	Horizon	Blue, "pear" shaped
36 30	,,	S.	
36 50	,,	,,	
37 5	,,	,,	
37 24	Regulus	,,	Long train
40 35	Below Regulus	,,	Small
41 50	Leo	,,	
42 0	,,	,,	
42 3	,,	,,	maximum
42 12	,,	Horizontal to S.	
42 12	,,	,,	
42 13	,,	Zenith	
43 30	,,	Horizon	
44 20	S. of Regulus	S.W.	Head, orange; train, blue an white. Very fine
45 57	Leo	Horizontal to S.	Bright
46 32	,,	S.	Orange
49 50	,,	,,	,,
49 50	,,	,,	
50 5	,,	N.	
50 50	,,	S.	
53 43	N. of ♂	Zenith	Head, orange; train, blue
54 10	Leo	Horizon	
55 12	,,	S.	
55 20	,,	,,	
56 10	,	Horizon	"Pear" shaped.
57 45	Below Regulus	S. W.	
57 52	,,	,,	
58 28	Under ♂	N.	
58 40	Leo	S.	Small
14 59 5	,,	,,	
15 4 7	ζ Leonis	Horizon	"Pear" shaped.
4 27	,,	,,	

Cape Mean Time. h m s	Direction. From	To	Remarks.
15 4 35	♂	N.	
6 5	Leo	Horizon	
7 40	,,	Zenith	
10 45	,,	Horizontal to S.	Very bright.
11 50	♂	N.	
12 5	♀	S.	
13 43	♀	Regulus	
13 50	Regulus	S. W.	
14 30	,,	S.	Long train.
15 15	,,	Zenith	,, orange.
15 50	Below ♂	Horizon	Large.
16 50	,,	S.	
19 30	,,	Horizon	
20 0	,,	N.	
21 35	,,	S.	Orange.
25 8	,,	N	
25 11	,,	,,	
27 1	ε Leonis	♂	
27 10	∂ Leonis	Horizon	
27 55	,,	N.	
28 0	,,	S.	
28 50	ζ Leonis	N.	
29 40	β Leonis	♀	
29 50	∂ Leonis	S.	
30 4	,,	W.	Red train.
30 10	α Hydræ	Horizon	
30 55	β Leonis	N. horizon	
34 10	β Corvi	Horizon	
34 30	∂ Leonis	,,	
36 10	η Leonis	β Corvi	Orange.
36 35	∂ Leonis	Horizon	
36 35	♂	W.	
37 10	Regulus	S. W.	Orange, large.
37 25	,,	α Centauri	Large, long train.
39 55	γ Leonis	W.	Orange and red, bright.
41 10	♂	Horizon	
41 30	η Leonis	β Corvi	Small.
41 50	ζ Leonis	S. W.	
41 55	Regulus	,,	
42 15	ζ Leonis	Horizon	
42 40	,,	S. W.	Orange blue.
43 5	β Leonis	S.	
15 43 25	Leo	E. Horizon	Orange, train green.

Cape Mean Time. h m s	Direction. From	To	Remarks.
15 43 30	Regulus	α Centauri	,,
43 50	,,	,,	,,
43 52	,,	,,	,,
46 15	δ Leonis	β Corvi	
46 17	,,	,,	
46 50	,,	,,	
46 55	,,	,,	
47 0	Regulus	α Centauri	Orange, large.
49 30	ζ Leonis	Horizon	Orange and green.
49 40	,,	,,	,,
50 30	δ Leonis	,,	Small.
51 10	,,	,,	Green, small.
51 15	,,	S.	,,
52 10	,,	β Corvi	Orange and Green.
53 50	ζ Leonis	Horizon	
55 25	,,	W.	,,
57 15	Regulus	S.W.	Small.
58 7	η Leonis	,,	,,
15 59 0	ζ Leonis	Horizon	,,
16 4 0	Regulus	S.W.	
7 40	δ Leonis	Horizon	Orange.
12 0	,,	Spica	Orange and Green.
26 40	Regulus	β Corvi	
16 27 20	δ Leonis	Spica	

(Total 134.)

Royal Observatory,
Cape of Good Hope, Nov. 17th, 1868.

On some points connected with the Rediscussion of the Observations of the Transit of Venus, 1769. By E. J. Stone, Esq.

At the Séance de l'Académie des Sciences, 1869, January 4, M. Faye brought forward a defence of M. Powalky's discussion of the observations of the transit of *Venus,* 1769. The weight which, irrespective of its merits, is necessarily attached to a paper read before so learned a body and by so distinguished an Astronomer as M. Faye, compels me at once to express the grounds on which I am unable to accept either the methods or the results of M. Powalky's discussion.

My first inquiry shall be respecting the foundation of M. Faye's remark, that M. Powalky "a eu le mérite d'élucider le premier la cause d'une erreur compromettante pour la science."

I will lay down at once a few propositions to which I shall have frequent necessity of appealing.

1. There are two phenomena at internal contact which were

observed in 1769. The so-called real internal contact, and the apparent internal contact. These phenomena were in the transit of 1769 separated *roughly* by about 16^s.

2. An observation of an apparent contact is not an observation of a real contact, and *vice versâ*.

3. In any *legitimately* conducted investigation, the use of an apparent contact for a real contact, or *vice versâ*, must lead to an erroneous value of solar parallax to the extent which an arbitrary change of about 16^s in the incongruous observation would affect the final result.

4. If observations have to be rejected because of their incompatibility with the general run of the discussion, then every such rejection enormously increases the presumption against the truth of the investigation which requires such rejection.

5. The absolute necessity of rejecting *one* good observation would, in itself, be conclusive evidence against the truth of the deduced value of solar parallax or the logic of the method applied.

M. Powalky attributes the difference between his value and that deduced by Encke to his employment of more accurate longitudes than those available to Encke. The reason why I am compelled to dissent from this assertion will best appear by a consideration of the observations made use of by M. Powalky at those stations where durations were observed. We shall thus clear ourselves entirely from the effects of these more accurate determinations of longitude.

Wardhus Station.

M. Powalky employs, for internal contacts, Sajnovics' observation at ingress, "Certissimus ut ajebat." At egress, Hell's observation, "videtur aliqua gutta nigra intra limbum Solis et Veneris ante contactum fieri."

We have here one violation of (3).

Kola.

The observations at this station are not employed by M. Powalky.

Hudson's Bay.

We have here two observed durations from real contact to real contact. M. Powalky employs both observations at ingress, which are well represented in his residuals. He rejects, on account of discordance, both observations at egress. The observed durations agree within 1^s. We have here two violations of (4) if not even of (5). M. Powalky's solution would make each of the observers to have caught the real contact at egress *about* 13^s too soon.

St. Joseph.

M. Powalky has used the three observations of Chappe, V. Doz, and Medina, as observations of real contact. He has, however,

only given to the three observations the weight of one. The residual of the mean of the three observations at ingress amounts to $-7^{s}\cdot5$.

Otaheite.

M. Powalky follows Encke in considering the observations of apparent contact of the "penumbra as dark or nearly as dark as the planet itself," as observations of real contacts.

He employs, at ingress, the observations of Green and Solander. He rejects Cook's observation, with a residual of $-15^{s}\cdot6$. At egress, M. Powalky uses Green's observation with a weight only of $\frac{1}{2}$: the residual is $-7^{s}\cdot8$. He rejects Cook's observation, with a residual of $-17^{s}\cdot8$.

Now, upon this I would remark that, if the observations are observations of real contact, M. Powalky has here, by the rejection of Cook's observations and the little weight attributed to Green's observation at egress, violated (4), if not (5), several times. If these observations are not observations of real contacts, M. Powalky has violated (3) by using Green's observations. In these durations, be it remembered, we are free from the effects of longitude.

Of the ten complete durations from internal contacts (all that are available) which I have used in my paper and which are there, at least, perfectly represented, M. Powalky has employed, in their entirety, only four. To three of these combined he gives the weight of one only. The fourth duration, that at Otaheite, he partially rejects by giving only a weight of one-half to Green's observation at egress.

To these he has added a duration obtained from an ingress observation of Sajnovics' and an egress observation of Hell. This is the only duration well represented in his solution.

With respect to the observations at stations where durations were not observed I shall have but little to remark.

The selection of Hitchins' observation at $7^{h}\ 28^{m}\ 57^{s}\cdot6$ A.T. as the representative observation of real contact, in opposition to the whole body of observers in England and Southern Europe, appears to me unsound. If we know better than these observations can teach us, we need not appeal to them, but if we do appeal to them for information we must accept fairly the information they afford. The greater the discordances amongst the observations—the greater the difficulties under which the observations were made—the more unsafe must become any selection of an extreme value, like that of Hitchins', to the exclusion of the mean.

The selection of Shippen's observation as the representative observation of real contact for observations made with factor about 27, on the American continent, appears to me also objectionable. There are at least seven other observations on this station, all of which appear to give a later time than Shippen's for the disappearance of the connecting ligament at ingress.

An alteration in the value of solar parallax of 0″·4 is a question only of less than 2ˢ between Wardhus and Greenwich, and of less than 7ˢ between Wardhus and Shippen's station, Philadelphia. The observations of Hitchins and Shippen, selected from groups, and each early and extreme observations of real contact, are given full weight. I think it will hardly escape notice how M. Powalky's solution fails, where the real strain falls upon it, to reconcile the St. Joseph, and more particularly the Otaheite observations with those made in the extreme northern stations. I must remark, M. Powalky has considered the question of adopted semi-diameter of so little importance in the discussion, that he has adopted Encke's value deduced from the employment of very different data.

We knew, before M. Powalky's paper appeared, that by adopting different data different results could be obtained. What we did not know was any cause for these discrepancies. The selection of the material had been based upon no intelligible principle, and no one had been able to reconcile the whole of the durations. I now leave M. Faye's defence of M. Powalky's work to the judgment of competent astronomers. I, at least, do not consider that "il a eu le mérite d'élucider le premier la cause d'une erreur compromettante pour la science."

I now pass to my own work upon this important question. In my opinion all the difficulties which have been experienced in the discussion of the observations of the Transit of *Venus* in 1769, are to be traced to a single source—mistaken notions with respect to the adoption of semi-diameter, and insufficient recognition of the nature of the uncertainties thus introduced. My view will perhaps be more clearly understood as follows. If we knew the true value of the solar parallax, the true R.A. and N.P.D. of the Sun and of the planet, the true semi-diameter of the Sun and planet, we could compute, with the utmost accuracy, the time of *real* contact for each station. If we knew the apparent semi-diameters of the Sun and of *Venus*, when projected upon the Sun's disc, and enough of the theory of irradiation, we could compute with the same accuracy the apparent internal contact; in fact, every phase of the phenomena presented from the real contact to the exterior contact. Now, the so-called observed real internal contacts which are the times of first appearance or disappearance of the connecting ligament (*i.e.* the times at which the observer first recognises the existence of the ligament) can no more, at present, be directly computed than the so-called apparent contacts. The two phenomena stand in much the same position. They are both complicated by the effects of irradiation. If we make use of durations, every observed duration, either from apparent contacts or real contacts, will lead to an equation of condition of the form $\frac{x_1+y_1}{2}+a_1\,d\pi =$ observed discordance; a_1 is a constant depending on the observer's position, $d\pi$ the correction to the assumed parallax, x_1 and y_1, two functions of errors of relative

tabular R.A. and N.P.D., and of *correction for semi-diameters, corresponding to the observed phase at ingress and egress respectively.*

Now the observer really observed *some phase.* If we knew the true values of x_1 and y_1, each equation would give the true value of $d\pi$. Unfortunately we know nothing about x_1 and y_1. Their values must be deduced from the observations themselves as approximately as we can find them. If we can find several observations which appear to be observations of the same phase, we can assume the corresponding x and y, one or both, to be the same for these several observations. The deviation of the phase really caught by the observer from our mean phase is now really the so-called error of observation, which is, in fact, nothing more than the error committed in assuming that the phase observed is the mean phase.

The attention of observers having been chiefly directed to observations of apparent contact and the so-called real contacts, the observations naturally group themselves around two mean phases near the time of real contact and the time of apparent contact respectively. The observations of these two phases differ by about 16^s. If we had enough observations, we might assume all these contacts, real and apparent, to be observations of the same phenomenon. The probable error of an observation measured by the residual errors would in this case be very large, for we include in these residuals all the errors made by assuming that the observations refer to the mean phase. A very large number of observations well distributed would thus be required in order that a result of any accuracy should be deduced. We clearly have not enough observations for such rough work.

If we advance one step beyond this, and assume that all the observations of real contact correspond to one mean phase, all the observations of apparent contact to another mean phase, we shall have made a considerable reduction in the probable error of each observation. We shall thus require, comparatively speaking, a small number of observations, even although we have thus introduced one more unknown quantity. This, of course, depends upon the attention of the observers having been directed to two essential points; the observer's attention having been directed to the real and apparent contacts, the observations naturally group themselves around two corresponding mean phases. I have, therefore, adopted two unknown quantities x and y; one corresponding to an apparent, the other to a real mean phase. And here a word or two on M. Faye's assumption, with respect to the accuracy which is thus to be obtained. That accuracy depends only, upon the factors of parallax correction involved in the equations of condition and the closeness with which the observations of apparent and real contacts group themselves around their respective mean phases. The accuracy really obtained can be judged of solely by the smallness of the final residuals. It may, perhaps, be said, that in thus

introducing one more unknown quantity, the final residuals must necessarily be smaller than those otherwise obtained. To this I reply that the new unknown quantity which I have introduced amounts to very little more than the arbitrary selection of observations which were assumed to be real internal contacts at those stations where both apparent and real contacts were observed, a thing which has always been previously done. At all events, I use both apparent and real contacts, and I have introduced no more unknown quantities than are absolutely required.

There is one important point that must here be borne in mind. The x and y introduced in my equations of condition are determined by making the residual errors the least possible. This is the analytical consideration from which they are determined. But x and y have been introduced to correspond to the observed mean phases of apparent and real contact. They have, therefore, a physical interpretation. We must, therefore, have, if our solution is a true solution, $\frac{x-y}{2}$ equal to the differences in time between these two mean phases. Now, amongst the observations at the five stations at which durations have been observed, some three differences between the two phases may be collected. Their mean value agrees so closely with that deduced from my equations that all these mixed observations might have been included without changing my result. I look upon this point as one of the utmost importance. Had the solution been a forced solution the probability of such an agreement would be small indeed.

The ten durations are all satisfied. The necessary condition is satisfied by the value of $\frac{x-y}{2}$. What reasons, therefore, have we to doubt the result? It must be remembered by M. Faye that the onus *now rests upon him* of proving, and not asserting, that some one of my observations has been erroneously referred to mean phase in such a way as to seriously affect my results; or that there are observed durations which are without doubt correct, and which are inconsistent with my solution.

Now to what extent has M. Faye been enabled even to throw doubt upon my distribution of observations? With respect to the Wardhus observations, he *thinks* that "fulmen $32^m\ 48^s$" is an observation of real contact at ingress, and he would prefer using this and Hell's observations of real contact at egress to using apparent contacts. Now the question is, are we certain that fulmen $32^m\ 48^s$ represents any attempted observation of real contact? I doubt it for the following reasons:—Professor Littrow states that "fulmen $32^m\ 48^s$" only occurs in the darker ink in which some corrections and explanatory words were afterwards added. In the fac-simile of this important part of Hell's Journal, which Professor Littrow has done me the honour to forward to me, I

find this observation completely barred off from Hell's observations; in fact, cramped in at the top of Sajnovics' observations.

I remark also that the words employed in describing the phenomena at ingress are similar to those employed in describing the corresponding phenomena at the egress. We have a very clear description of the phenomena observed at the real contact at egress. The description is certainly very different indeed from the laconic use of a single word, *fulmen*. I believe that this most probably refers to that apparent flowing in of the light upon both sides which is so clearly described by Wales at the Hudson's Bay observations of ingress. "We took for the instant of the first internal contact the time when the least visible thread of light appeared behind the subsequent limb of *Venus*, but before that time *Venus's* limb *seemed within that of the Sun.*"

I do not say that this is the true explanation of the introduction of these words, but I do say that "fulmen $32^m\ 48^s$" is not entered as an observation like the others in due order. It is completely barred off from Hell's observation, and its meaning is exceedingly doubtful. Having at this station two complete observations of apparent contacts, I rest my discussion on this sure basis in preference to the introduction of materials so doubtful as the time opposite this entry of *fulmen.*

It may perhaps be thought that this is a critical point in my argument. Such, however, is not the case. If I had used the "fulmen $32^m\ 48^s$," as an observation of real contact, my results would have been but little modified. Take it as an observation of real contact, which I do not however believe it to be, and combine with Hell's observation of real contact at egress, and we have a duration which is represented with a residual error of about 9^s *by my solution:* a discordance not large enough to alter much the resulting parallax, but large enough to justify the refusal to introduce so doubtful an observation. I preferred, and I still prefer, to use the more certain materials of the apparent contacts. With respect to the accuracy with which these apparent contacts can be observed, the discordance of Hell's and Sajnovics' observations is only $4^s.5$.

With respect to the St. Joseph observations, I have so fully discussed this point in my answer to Mr. Newcomb, that I shall say but little here. I believe Chappe's words; and Pauly's observation of some contact at egress, 22^s before Chappe's observation, *proves* that Chappe did not observe a real contact or the *first* appearance of the connecting ligament. I believe it proves *à priori* that he really caught some phase much nearer the mean phase of apparent contacts. If however there is any doubt upon the point, appeal must be made to the residuals. Now with my interpretation all these residuals, and all other residuals, are in beautiful accord. This is admitted. On M. Faye's assumption all the residuals are in discord. I am really quite unable to understand M. Faye's inference from the fact that, if we arbitrarily change the interpretation of the egress observa-

tions at St. Joseph from apparent to real contacts, we throw the residuals which were all before in agreement into disagreement. M. Faye infers that this proves that the agreement of all the residuals was before forced. I take it most clearly to prove that M. Faye's interpretation is not the true one. If we knew the true values of the phase correction at ingress and egress there would remain in the residuals nothing but the error of assumed parallax, which, as my result differs only by $0''.05$ from that which M. Faye wishes to be considered the truth, would amount to less than 2^s. Moreover, with M. Faye's interpretation, we have all the other residuals thrown into confusion, and particularly the Hudson's Bay observations, which are undoubtedly observations of real contacts, each saddled with a discordance of the same sign, and amounting to $7^s.9$ and $8^s.9$ respectively. We have, moreover, with M. Faye's interpretation, two values of solar parallax $8''.91$, identical with my own, from the apparent contacts, and $8''.70$, from M. Faye's attempt to improve the real contact observations. I must here, as before, recall to M. Faye's mind that, *cæteris paribus*, harmony in the results is a proof of truth. If he denies the truth, he must prove, not assume, that I have referred my contacts to a wrong mean phase, or that there are well-observed durations incompatible with my solution.

I next pass to the Otaheite observations. I have taken the apparent contacts of the "penumbra as dark, or nearly as dark, as that of the planet itself," to be apparent contacts. M. Faye admits that they are not real contacts. He thinks however, that they are some phase between real and apparent contacts. Such an impression can only be tested by the agreement or disagreement of the results. Such a test is dead against M. Faye's impression. The observations are all in agreement. But what reasons does M. Faye bring forward that render such an impression even plausible? He thinks that the form of the reflectors made use of by all the observers must have been distorted, so that the image of a point was no longer a point. The observers made no mention of any want of definition. M. Faye then assumes that such want of figure, which would undoubtedly injure definition, would seriously interfere with an observation of apparent contact when the limb of the planet and of the Sun are similarly reflected. I have not the slightest belief in the production of such an effect, to any important amount, even if the cause did exist. M. Faye must remember that I have allowed 3^s for probable error of each contact. We are not, fortunately, in this inquiry concerned with $0^s.5$ of a second. My own explanation of this penumbra is that it is nothing more than the fuzzy light which appears around the edge of a dark body projected on a bright body like the Sun. I saw a similar phenomenon at the last transit of *Mercury*. The observers in 1769 were impressed with the opportunity presented by a transit of *Venus* to detect the presence of an atmosphere; any such appearance of

stray light about the edges of *Venus* would, if it once attracted attention, be naturally deemed of some importance. The name penumbra is by no means an unsuggestive name for such an appearance to any one who did not attribute the appearance to its true cause, irradiation.

Such are the points in which M. Faye takes exception to my work.

He thinks that I ought to have used something, we know not what, as an observation of real contact at Wardhus. Had I used this it would have but little modified my result. It may be an observation of real contact: it may be, as I have suggested, an indication of the appearance of light nearly all round the planet. I only say that it is not inconsistent with my solution in any case, but that I prefer to rest the solution upon a more intelligible basis. M. Faye prefers to use Chappe's observation at egress as a real contact. I say that Pauly's observation shows this to be inadmissible, and the residuals say the same. Chappe's own words, in my opinion, tend to the same conclusion. M. Faye thinks that the Otaheite observations of apparent contacts of penumbra are something between a real and an apparent mean phase. The residuals say what the observer's own words appear to indicate, that they are observations of apparent contact. Assume that, in all the cases specified, M. Faye's interpretations were as simple and as reasonable as my own (I am very far, indeed, however, from granting this) — what then? my assumption leads to perfect harmony amongst the observations themselves. M. Faye throws all again into chaos. M. Faye has certainly overlooked the fact that the observers did observe something. The so-called errors of observation would be reduced to nothing if we only knew the proper corrections to reduce all the observations to the same phase. With respect to the probable error of my result, I have assumed the probable error of the deviation of such observations from the corresponding mean phase to be 3^s. The probable error of the resulting parallax is then less than $0''{\cdot}03$. All I can say is, that the residuals do not appear to require a larger assumption of probable error. However, if we assume 6^s, the probable error of the result would be only be about $0''{\cdot}05$. The very form which all discussions are now taking upon transits of *Venus* and transits of *Mercury;* questions of irradiation; questions whether observations refer to real or apparent contacts; separation, justly enough, of observations of contacts of *Mercury* into classes; shows the flood of light which has been let into this question since the appearance of my paper. Transits of *Mercury* have taken place before: the phenomenon of the black drop was known; but it was not known that the value of the solar parallax which resulted from the discussion of the observations of the transit of 1769 was entirely a question of the reference of a few observations to apparent or real contacts. It was not known that by interpretations put upon the observer's words, at least as simple as any interpretations which can be put upon them, the whole of the observations collected could

be reduced into perfect harmony. And that the value of the solar parallax which will *alone* reduce these observations to harmony is a value so close to what we now believe the truth, that no one method, and probably not the mean of all the other methods, can say how much that error may be. I leave my work with confidence to the judgment of astronomers, knowing that justice will be fully done, and whatever my real claims may be they will ultimately be fairly recognised.

It may perhaps be desirable that attention should be called to the agreement which holds between the results of my paper "On the Rediscussion of the Transit of Venus, 1769," and the numerous observations of ingress obtained in England, Southern Europe, and on the American Continent; also with a few scattered observations of egress. These observations were not employed in my discussion. Their introduction would not, in my opinion, have added sensibly, if at all, to the true weight of the final result. It is however of course necessary that my solution, if a true solution, should not be inconsistent with their evidence. This point had received my most serious consideration before the appearance of my paper. I considered the evidence thus afforded was confirmatory of my general solution. I would remark that the observations which I have made use of are the best for several reasons; they are free from questions of absolute time, a point of paramount importance. The observers at these stations all used, so far as we have any information on the point, considerable magnifying powers, and, what is of still more importance, powers very nearly equal. From the importance of the observations at these selected stations we may reasonably suppose that their instrumental equipments were the best available at the time. From their drawings and verbal descriptions their attention appears to have been drawn to the observations of the same two principal points. These observations were made under fair atmospherical conditions. We cannot, therefore, expect to have so good an agreement amongst miscellaneous results, collected under unfavourable circumstances, or with very different powers and complicated with errors of absolute time, as amongst the results made under especially favourable conditions and entirely free from all systematic errors except the rates of their clocks.

The observations made in England and Southern Europe were all made under most unfavourable circumstances. The Sun at Greenwich was less than 5° high at internal contact, and lower considerably in France, most of the observations having been made at a less altitude than $3\frac{1}{2}°$. Many of the observations were made with low powers; the results are all complicated by errors of absolute time. The attention of the observers appears to have been directed to very different methods of estimating the time of real contact; some even waited for the time when the disk of *Venus* appeared again perfectly round,—a very rough observation, I presume, at such low altitudes. Under such circumstances

great discordances might be expected. The observations are, in fact, so discordant that only one method appears to me available for making any use of them. If we collect all the observations for which the factor of parallax is nearly the same, and which appear or profess to be observations of apparent contact, and take their mean; and if we proceed exactly in the same manner with the observations of real contact, the mean of the two results thus obtained should correspond with some accuracy to a phase half-way between the apparent and mean phases. This method may appear rough, but the materials are rough, and no objection can be urged against its fairness. From 89 observations thus available I have found a residual of $+ 22^s$. My method may perhaps be better understood if we confine our attention to the observations made at the Royal Observatory, Greenwich. We have here four observations of "Regular circumference of Sun and *Venus* in contact." The mean apparent time is $7^h\ 28^m\ 45^s$. We have five observations of "Completion of thread of light." The mean is $7^h\ 29^m\ 23^s$ A.T. The mean of these times, $7^h\ 29^m\ 4^s$ A.T., should correspond to the mean phase very nearly. The residual for this time for this station is $+ 25^s$, agreeing pretty closely with the general mean. I will now deduce from my general solution what should be the residual for mean phase at Greenwich, assuming the mean of Hell's and Sajnovics' observations of apparent contact at Wardhus to be free from error. We have from these observations,

$$35^s + 10{\cdot}24\, d\alpha - 16{\cdot}24\, d\Delta - 44{\cdot}5\, d\Pi - 19{\cdot}7\, \varrho = 0.$$

If A and T are the residuals for apparent contact and true contact at Greenwich,

$$A + 10{\cdot}14\, d\alpha - 16{\cdot}36\, d\Delta - 48{\cdot}4\, d\Pi - 19{\cdot}7\, \varrho\, d\Pi = 0.$$

or sensibly, since $d\Pi = 0''{\cdot}42$,

$$A = 35 + 3{\cdot}9\ d\Pi = 36{\cdot}6$$
$$T = A - 16{\cdot}6 \qquad = 20{\cdot}0.$$

Consequently

$$\frac{A + T}{2} = 28^s{\cdot}3.$$

The quantity obtained from the Greenwich Observations is 25^s.

I think this agreement is sufficient to show that the Wardhus Observations and the Greenwich Observations are reconciled by my solution. The residual for Greenwich, 25^s, is rather larger than the general mean, but this general mean is drawn down by some observations made in the South of France, where the circumstances of observation were still much more unfavourable than at Greenwich, as stated above.

I shall next pass to the consideration of the numerous observations made on the North American Continent. These observations were made under fair atmospherical circumstances, and

their agreement is good, especially when free from uncertainties of absolute time. The instrumental equipments of some of the observers were, however, rather imperfect, and uncertainties still hang over the determinations of the longitudes of some of the stations, and even of the exact position of some of the observing stations. Here, again, I shall lay more stress in representing the mean of the observations, than in representing with great accuracy one selected observation.

The following residuals are extracted from Encke's discussion, with corrections to assumed longitudes extracted from a table in M. Powalky's discussion.

Station.	Observer.		
Philadelphia	Shippen	$+21^s$	real contact.
,,	Williamson	30	apparent contact.
,,	Thomson	27	
,,	Prior	29	
,,	Erving	31	

Shippen states that he tried to observe the completion of the thread of light. I shall take, therefore, Shippen's to be an attempt at a real contact, and the other observations as attempts at apparent contacts.

Station.	Observer.		
Norriton	Lukens	$40^s\cdot9$	apparent contacts.
,,	Smith	$37^s\cdot9$	

The description of the phenomena observed is not very clear, but the observations appear to me to be attempts at apparent contacts ; Ritterhouse observed, at the same station, some connexion long after the times given for these contacts, which can therefore, only be assumed to be attempts at apparent contacts.

Cambridge.

Professor Winthrop, $+9^s\cdot2$. Real contact.

This observation is of great value. The observer's position is well known. He took great care to determine the time with accuracy.

Station.	Observer.		
Providence	West	$+1^s\cdot0$	real contact ?
Wilmington	Poole	$+3\cdot9$	real contact.
Lewes	Biddle	$-9\cdot0$	real contacts.
,,	Bailey	$-5\cdot0$	
Newbury	Williams	$-0\cdot3$	real contact.
Baskeridge	Stirling	$-0\cdot0$	real contact.

These results would give the following residuals,—

For apparent contacts	+32·6	mean 17ˢ·6.
real contacts	+ 2·6	

The residual for the real contact is made small by the observations at Lewes, and is, of course, affected by any error in the assumed longitude ; no observations are given to test the accuracy with which the time was determined.

Then, assuming as before the Wardhus observation to be correct, I deduce from my solution for the mean station of these observations

For apparent contacts	+28·1	mean 19ˢ·8.
real contacts	+11·5	

The mean results of calculation, based upon the Wardhus observations and my solution, and the results of the observations of these fourteen observers, agree within about 2ˢ. The residual for real contact from my solution is almost identical with the means of Shippen's, Winthrop's, and Poole's results.

If the means of these American observations are taken without any attempt to discriminate between the phases, the residuals cannot be expected to agree so closely. The difference is, however, only about 5ˢ. There is a most valuable group of observations made at St. Domingue by Pingré, Fleurieu, La Filière, and Destourès. The observations are entered as

" *Contact intérieur des bords du Soleil et de Vénus.*

Temps vrai. h ′ ″	
2 44 45	Selon M. de Fleurieu.
2 44 41	,, M. le Chevalier de la Filière.
2 44 50	,, M. Destourès.
2 44 44	,, M. Pingré."

I assume these observations to be those of apparent contact or of contact of limb with limb. Assuming, as before, the Wardhus observations as correct, I find that the mean of these results is represented within 1ˢ by my general solution. If we assume the mean of the Otaheite observations to be true, we can represent these observations within 3ˢ.

With respect to the egress observations, if we take the mean of Green's and Cook's observations to be the true time of apparent contact at Otaheite, and assume the results of my re-discussion, we can represent the observations with factor about + 42·6 $d\,\Pi$, viz. the real contact observations of Lowitz's, Inochodsow, and Kraft's at Gurief and Orenbourg within 5ˢ; the mean of Mohr's and Dollières' observations at Batavia and Pekin, with factor about + 28·4 $d\,\Pi$ within about 4ˢ; the apparent contact of Collas, at Pekin, within about 2ˢ. These are really all the checks which we can apply to the Wardhus and Otaheite

observations respectively which are not applied in my re-discussion, and these checks appear to me all confirmatory of the interpretations I have put upon the Wardhus and Otaheite observations.

The only point which does not appear to me perfectly confirmatory of my solution is the apparently rather close agreement of the Hudson Bay observations of ingress, which are undoubtedly real contact observations, with the apparent contacts at Wardhus. Upon this I can only remark that the longitude of this station depends upon the discussion of a few occultations made nearly one hundred years ago; that this difficulty arises entirely from the great difference in the longitude deduced from these same occultations by M. Powalky from that previously deduced by Encke; that M. Powalky's longitude makes the egress observations entirely disagree with other undoubted observations of real contact; *that the durations at this station are perfectly represented in my solution.* The uncertainties still resting on the longitudes of this station and of St. Joseph afford the best justification of the method I adopted in making the discussion depend solely upon durations. I am more than ever convinced of the extreme importance of observing the so-called apparent contacts as well as the so-called real contacts. It will be seen from what precedes that a mean between apparent and real contacts is a phase represented with accuracy. In conclusion, I may, perhaps, be allowed to suggest that discussions which reject the observations at *Otaheite or Wardhus and Kola* as inconsistent with their results are not of very much weight. The strain on the truth of the result is in these cases really slipped, not borne.

My object in this addition is simply to afford independent proofs that the adopted times for Wardhus and Otaheite are not much in error; if these observations are sensibly correct, there can be no doubt about the value of solar parallax.

Addition to Mr. Proctor's Paper on the Transit of Venus in 1874. (*Monthly Notices for March 12th*, 1869.)

(*Communicated by Mr. Proctor.*)

The four following corrections refer to printer's errors:—

Page 213, in the expression for V, $d'^{\frac{3}{2}}$ *should be* $d'^{\frac{1}{2}}$.

Page 215, line 14 from bottom, *for* elliptical *read* ecliptical.

Page 217, line 9 from top, *for* l' *read* λ'.

Page 217, line 8 from bottom, *for* 54° 27′ *read* 44° 27′.

The last correction is important, though the context (*see* p. 218, line 12) sufficiently shows what was intended.

I have gone very carefully through the calculations on which the results in my paper were founded, and have detected a slight error affecting the last three longitudes in the table on p. 217. The error (one of transcription) does not invalidate, but on the contrary rather strengthens, the conclusions arrived at in my paper. As the first of the three longitudes just named was also wrongly printed, it seems best to give the whole table, as corrected :—

	Lat.	Long.
(i.) The place at which first internal contact is most accelerated lies in	° ′ 39 45 N.	° ′ 143 23 W.
(ii.) The place at which first internal contact is most retarded lies in	44 27 S.	26 27 E.
(iii.) The place at which last internal contact is most accelerated lies in	64 47 S.	114 37 W.
(iv.) The place at which last internal contact is most retarded lies in	62 5 N.	48 22 E.

I have calculated the positions of the corresponding places with the elements given by Mr. Airy in the *Monthly Notices* for December 11th, 1868, p. 34. These elements are not strictly congruous, it will be noticed, but I use them without change ; except that I have ventured to alter the value of the Sun's declination, which is obviously incorrect. The places corresponding to (i.) and (ii.) lie respectively in north and south latitude 37° 13′ and in longitude 138° 38′ W. and 41° 22′ E., respectively. The places corresponding to (iii.) and (iv.) lie respectively in north and south latitude 60° 1′, and in longitude 136° 11′ W. and 43° 49′ E., respectively. The distances of these four places from those tabulated above I find to be (by the method of mid-latitudes) 314·0, 920·2, 764·5, and 208·7 miles, respectively.

On some Effects of the Comparative Clinging of the Limb of Venus to that of the Sun in the Transit of 1874, *as compared with that of* 1882. By E. J. Stone, Esq.

There is one point of great practical importance which must not be overlooked in estimating the relative values of the Transits of *Venus*, 1874 and 1882, and in determining which of the methods proposed for utilizing the Transit of 1874 is the more valuable. The chord described in 1874 is considerably less than the corresponding chord in 1882. M. Puiseux has shown, however, that very considerable differences of duration can be obtained in the Transit of 1874, and his results have been confirmed by Mr. Proctor. The same cause, however, which renders this possible renders the observations themselves less accurate. The object of this short Note is to obtain an estimation of the injurious effects

upon the observations themselves arising from the slow motion of *Venus* across the Sun's limbs, and to trace the consequences upon the methods proposed by the Astronomer Royal and M. Puiseux for utilizing the Transit of 1874. The difficulty in deducing a value of Solar Parallax from observations of contacts of *Venus* and the Sun is that of referring the observations to the same phase. We are really, therefore, concerned with estimations of angular measures by their equivalent times. I find from Mr. Hind's calculation, *Comptes Rendus*, 1861, July 22, that, with the same assumed semidiameter of *Venus*, the time from external contact to internal contact in the Transit of 1874 is no less than 29^m, whilst in that of 1882 it is only $20^m\ 19^s$. From this I conclude that, if e be the probable error in time of an observation in 1882, the corresponding probable error for the Transit of 1874 will be

$$e \times \frac{29^m}{20^m\ 19^s} = e\,\frac{1740}{1219}.$$

A difference of durations of $20^m\ 19^s$ is, therefore, in 1882 as valuable as one of 29^m in 1874. Other differences are effective in like proportion. It appears to me impossible to secure good observations in 1874 with such differences as those mentioned by Mr. Proctor. It must be remembered that it is useless to discuss differences obtained from the consideration of stations at which the necessary observations cannot be made on account of the small altitudes of the Sun at the times of internal contacts. For my part I should at once exclude from consideration all stations with altitudes of the Sun, at either ingress or egress, much less than 10°. I consider even this too low, unless a good deal has to be sacrificed in parallax-factor to secure greater elevation.

How small the value of observations made within 5° of the horizon is, will at once appear by a comparison of the English and French observations of the Transit of 1769 with other observations made at greater altitudes of the Sun.

The relative effects of the increased probable error of internal contacts in 1874, on the method proposed by the Astronomer Royal and M. Puiseux, is seen from the following formulæ:—

Let l be the probable error of a longitude determination,
e that of an internal contact,
then (constant)^2 (probable error of parallax)^2

$= 4e^2$. In the method of durations.
$= 2(e^2 + l^2)$. In Mr. Airy's method.

the relative weights are therefore

$$\frac{\text{Puiseux}}{\text{Airy}} = \frac{(\text{Puiseux constant})^2}{(\text{Airy constant})^2} \cdot \frac{1}{2}\left\{1 + \frac{l^2}{e^2}\right\}$$

Since, therefore, l is constant for all times, and e is greatly

increased in 1874, we see at once from the formula that the method of durations contrasts much more unfavourably with that of absolute time determination in 1874, than in that of 1882 or in that of 1769. This is probably the ground upon which the Astronomer Royal still gives his preference to absolute time determinations for the Transit of 1874. The formula can very approximately be turned into numbers. The Astronomer Royal considers that we are now in a position to determine longitudes with a probable error of 1^s. In my re-discussion of the Transit of *Venus*, 1769, I have assumed 3^s for the probable error of an internal contact. This appears to cover the residuals, but it has been considered too small by some astronomers. Taking it, however, at 3^s for 1882, we must assume $e = 4^s{\cdot}28$ for 1874. In order then that the probable errors of M. Puiseux's method and that of Mr. Airy should be equal, we must have differences of duration of 31^m in M. Puiseux's method to correspond to differences of 22^m in Mr. Airy's. This of course depends upon the possibility of determining longitudes within 1^s. I do not think that the probable error of internal contacts has been over-estimated.

The method proposed by the Astronomer Royal possesses also another advantage; it removes the necessity of attempting to push observers into the Antarctic regions. Kerguelen's Islands and the Macdonald Islands are, however, favourable positions for both methods, and there appears every reason to avail ourselves of both methods to at least this extent. From the slow motion of *Venus* from the Sun's limb, I do not consider the Transit of 1874 a favourable one.

Blackheath,
1869, *April* 8.

Occultations of Stars by the Moon and Phenomena of Jupiter's Satellites, observed at the Royal Observatory, Greenwich, from 1868, *January, to* 1869, *March.* Communicated by G. B. Airy, Esq., Astronomer Royal.

Occultation of Stars by the Moon.

Day of Obs. 1868.		Phenomenon.	Moon's Limb.	Mean Solar Time. h m s	Observer.
Feb. 28	(*a*)	μ Ceti, disappearance	Dark	9 24 45·7	J.C.
		μ Ceti, reappearance	Bright	9 47 51·1	J.C.
Mar. 1		θ^2 Tauri, disappearance	Dark	6 38 24·5	E.
		θ^1 Tauri, disappearance	Dark	6 38 37·0	E.
May 4	(*b*)	l^2 Virginis, disappearance	Dark	9 7 35·6	C.
	(*c*)	l^2 Virginis, reappearance	Bright	10 21 36·3	C.

Day of Obs. 1868.		Phenomenon.	Moon's Limb.	Mean Solar Time. h m s	Observer.
May 27		18 Leonis, disappearance	Dark	10 30 59·3	D.
Dec. 27 1869.	(*d*)	B.A.C. 1526, disappearance	Dark	6 27 51·2	J.C.
Jan. 24		119 Tauri, disappearance	Dark	8 14 33·3	D.
		120 Tauri, disappearance	Dark	8 47 42·5	D.

(*a*) Cloudy.
(*b*) The disappearance did not seem to take place so suddenly as usual.
(*c*) Thought to be a little late.
(*d*) Uncertain to a second; clock beats scarcely audible from high wind.

Phenomena of Jupiter's Satellites.

Day of Obs. 1868.	Satellites.		Phenomenon.	Mean Solar Time. h m s	Observer.
Jan. 15	III.		Occult. reappearance, first contact	6 4 15·6	G.K.
			,, ,, bisection	6 5 34·4	G.K.
			,, ,, last contact	6 6 5·3	G.K.
			Eclipse, disappearance	6 16 53·5	G.K.
Feb. 1	I.	(*a*)	Eclipse, reappearance	5 44 2·5	J.C.
July 13	II.	(*b*)	Eclipse, reappearance	12 14 28·6	J.C.
	II.	(*c*)	Occult. disappearance, bisection	12 25 16·9	J.C.
19	III.		Eclipse, disappearance	14 52 26·2	H.C.
23	III.	(*d*)	Transit, egress, last contact	13 14 29·8	C.
Aug. 24	I.		Eclipse, disappearance	10 42 4·6	C.
Sept. 4	III.	(*e*)	Transit, egress, bisection	10 35 40·6	H.C.
	III.		,, ,, last contact	10 40 24·8	H.C.
10	I.		Transit, egress, first contact	9 1 31·9	L.
	I.		,, ,, bisection	9 3 1·6	L.
	I.		,, ,, last contact	9 5 1·3	L.
24	I.		Transit, ingress, last contact	10 18 43·4	D.
	I.		Transit, egress, bisection	12 27 52·5	D.
	I.		,, ,, last contact	12 31 22·0	D.
29	III.	(*f*)	Occult. reappearance, last contact	9 59 15·1	C.
Oct. 1	II.		Transit, egress, bisection	10 58 48·6	J.C.
11	I.		Eclipse, reappearance	7 48 7·0	D.
16	I.		Occult. disappearance, first contact	12 37 12·8	C.
	I.		,, ,, bisection	12 39 27·4	C.
17	II.		Occult. disappearance, bisection	7 48 19·6	D.
	II.		,, ,, last contact	7 50 19·3	D.
24	III.		Transit, egress, last contact	9 49 33·3	E.
	II.		Occult. disappearance, first contact	10 0 31·5	E.
Nov. 2	I.		Transit, ingress, bisection	7 52 47·3	C.
	II.		Transit, egress, last contact	9 20 3·1	C.
	I.		,, ,, first contact	10 0 26·5	C.
	I.		,, ,, last contact	10 3 55·9	C.

Days of Obs. 1868.	Satellites.		Phenomenon.	Solar Solar Time. h m s	Observer.
Nov. 18	III.		Eclipse, disappearance	11 27 43·2	E.
Dec. 3	I.		Eclipse, reappearance	10 15 33·5	D.
11	I.	(*g*)	Transit, egress, last contact	8 9 1·4	C.
	II.	(*h*)	Transit, ingress, last contact	8 26 28·6	C.
12	I.		Eclipse, reappearance	6 40 56·0	E.
19	I.		Eclipse, reappearance	8 36 21·3	J.C.
31	III.		Occult. disappearance, bisection	5 59 37·8	J.C.
	III.		Occult. reappearance, last contact	8 58 8·9	J.C.
1869. Jan. 4	I.		Eclipse, reappearance	6 57 46·0	S.
5	II.		Transit, egress, last contact	8 7 16·7	C.
Feb. 5	III.		Eclipse, disappearance	7 59 4·4	E.

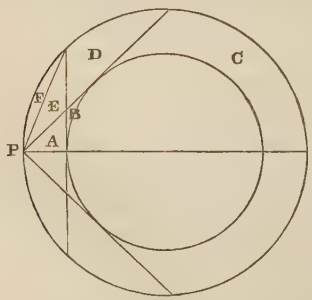
(*a*) (*d*) The image very bad.
(*b*) Uncertain, the planet being badly defined, and the satellite reappearing very near it.
(*c*) The planet very low and the obervation uncertain.
(*e*) The image very tremulous; the observation uncertain.
(*f*) Doubtful, in consequence of the planet being obscured by cloud every few moments.
(*g*) Very uncertain; the image of the planet very tremulous; clouds passing.
(*h*) Very tremulous.

The initials S., D., E., C., L., J.C., H.C., and G.K., are those of Mr. Stone, Mr. Dunkin, Mr. Ellis, Mr. Criswick, M., Lynn, Mr. Carpenter, Mr. H. Carpenter, and Mr. Keating.

Note on the Attraction of Ellipsoids.

By Prof. Cayley.

If an indefinitely thin shell of uniform density, bounded by two similar and similarly-situated ellipsoids, attracts a point P on its outer surface, it has been shown geometrically by M. Chasles that the attraction is in the direction of the normal at P, and is equal to twice the attraction of an infinite plate, the thickness of which is equal to the normal thickness at P. Assuming that the attraction is in the direction of the normal, the proof is in fact as follows:—with P as vertex, circumscribe to the interior surface a cone; this divides the shell into three parts; the one, D + E + F, exterior to the cone, the other two, A + B and C, interior to the cone. It is shown that in the direction of the



normal the attraction of C is equal to that of A + B; and it is assumed that in comparison with these the attraction of D + E + F may be neglected; the whole attraction is thus equal to twice that of the portion A + B. At the point where the normal at P meets the internal surface draw the tangent plane to the internal surface, thus dividing the portion A + B into the solid cone A and a remaining portion B; it is assumed that in comparison with that of A the attraction of B may be neglected; the whole attraction is thus equal to twice the attraction of the solid cone A; and the attraction of this solid cone is in the limit (the aperture or solid angle then becoming $= 2\pi$) equal to the attraction of an infinite plate whose thickness is equal to the altitude of the solid cone, that is, to the normal thickness at P. And the attraction of the whole ellipsoidal shell is thus ultimately (that is, when the shell is indefinitely thin) equal to twice the attraction of the infinite plate.

It is interesting to ascertain the orders of magnitude of the attractions of the several portions of the shell, which attractions are compared in the foregoing investigation; and this can be done very easily, when, instead of the ellipsoidal shell, we have a spherical shell (bounded by two concentric spherical surfaces). The tangent plane to the inner surface divides the portion D + E + F into two portions D and E + F; and if with P as vertex we describe a cone standing on the circle in which the tangent plane meets the outer surface, the last-mentioned portion is hereby divided into the portions E and F; the whole shell is thus divided into the portions A, B, C, D, E, F, each of them symmetrical in regard to the normal or radius at P, and consequently attracting in the direction of this radius. I proceed to find the attractions of each of these portions; it will appear, in accordance with the assumptions of the foregoing investigation, that, taking the radii to be 1 and $1 + \alpha$, that is, α the thickness of the shell, and supposing ultimately α to become indefinitely small, the attractions of A and C are each ultimately $= 2\pi\alpha$, that is $=$ to the attraction of the infinite plate, while the attractions of the other portions are of the order $\alpha^{\frac{3}{2}}$, and thus vanish in comparison with that of A or C.

The attraction of an indefinitely thin cone or frustum of a cone, length r and solid angle $d\omega$ is $= r\,d\omega$; considering any such cone having P for its vertex, if the inclination of r to the radius through P is $= \theta$, and if the azimuth of the plane through r and the radius is $= \phi$, then we have $d\omega = \sin\theta\,d\theta\,d\phi$, the attraction $r\,d\omega$ is $= r\sin\theta\,d\theta\,d\phi$, and this attraction resolved in the direction of the radius is $= r\sin\theta\cos\theta\,d\theta\,d\phi$. For the several cases which have to be considered, the value of r is independent of φ, and the integration in regard to ϕ is always from $\phi = 0$ to $\phi = 2\pi$;—the attraction is thus in each case $= 2\pi\int r\sin\theta\cos\theta\,d\theta$, the expression of r in the terms of θ, and the limits of θ being known for each of the several portions of

the shell. Taking θ_1 for the semi-angle of the tangent cone, we have it clear

$$\sin\theta_1 = \frac{1}{1+\alpha},\quad \cos\theta_1 = \frac{\sqrt{2\alpha+\alpha^2}}{1+\alpha};$$

and taking θ_2 for the semi-angle of the cone which divides the portions E, F,

$$\tan\theta_2 = \sqrt{\frac{2+\alpha}{\alpha}},\quad \sin\theta_2 = \frac{\sqrt{2+\alpha}}{\sqrt{2\,(1+\alpha)}},\quad \cos\theta_2 = \frac{\sqrt{\alpha}}{\sqrt{2\,(1+\alpha)}}.$$

For F we have

$$r = 2\,(1+\alpha)\cos\theta,\quad \theta=\theta_2 \text{ to } \theta=\frac{\pi}{2},$$

Integral is

$$= 2\,(1+\alpha)\int \sin\theta\cos^2\theta\,d\theta = \frac{2}{3}\,(1+\alpha)\cos^3\theta_2.$$

For D + E we have

$$r = 2\,(1+\alpha)\cos\theta,\quad \theta=\theta_1 \text{ to } \theta=\theta_2,$$

Integral is

$$= 2\,(1+\alpha)\int \sin\theta\cos^2\theta\,d\theta = \frac{2}{3}\,(1+\alpha)(\cos^3\theta_1 - \cos^3\theta_2).$$

For E, we have

$$r = \frac{\alpha}{\cos\theta},\quad \theta=\theta_1 \text{ to } \theta=\theta_2,$$

Integral is

$$= \alpha\int \sin\theta\,d\theta = \alpha\,(\cos\theta_1 - \cos\theta_2).$$

For A we have

$$r = \frac{\alpha}{\cos\theta},\quad \theta=0 \text{ to } \theta=\theta_1,$$

Integral is

$$= \alpha\int \sin\theta\,d\theta = \alpha\,(1-\cos\theta_1).$$

For A+B we have

$$r = (1+\alpha)\cos\theta - \sqrt{1-(1+\alpha)^2\sin^2\theta},\quad \theta=0 \text{ to } \theta=\theta_1,$$

Integral is

$$= \int \left\{(1+\alpha)\cos\theta - \sqrt{1-(1+\alpha)^2\sin^2\theta}\right\}\sin\theta\cos\theta\,d\theta$$

$$= (1+\alpha)\left(-\frac{1}{3}\cos^3\theta\right) + \frac{1}{3\,(1+\alpha)^2}\left\{1-(1+\alpha)^2\sin^2\theta\right\}^{\frac{3}{2}}, \text{ between the limits.}$$

$$= \frac{1}{3}\left\{(1+\alpha)\,(1-\cos^3\theta_1) - \frac{1}{(1+\alpha)^2}\right\}.$$

and subtracting the above value of the integral for A, it at once appears that, for B, the integral is

$$= 2\pi \left\{ \alpha (-1 + \cos\theta_1) + \frac{1}{3}(1+\alpha)(1-\cos^3\theta_1) - \frac{1}{3}\frac{1}{(1+\alpha)^2} \right\}.$$

Hence, calculating the approximate values, and restoring in each case the omitted factor, 2π, we have

$$\text{Attraction } A = 2\pi\alpha - 2\sqrt{2}\,\pi\alpha^{\frac{3}{2}},$$

$$,, \quad B = \frac{2}{3}\sqrt{2}\,\pi\alpha^{\frac{3}{2}},$$

$$,, \quad C = 2\pi\alpha - \frac{4}{3}\sqrt{2}\,\pi\alpha^{\frac{3}{2}},$$

$$,, \quad D = \frac{4}{3}\sqrt{2}\,\pi\alpha^{\frac{3}{2}},$$

$$,, \quad E = 1\sqrt{2}\,\pi\alpha^{\frac{3}{2}},$$

$$,, \quad F = \frac{1}{3}\sqrt{2}\,\pi\alpha^{\frac{3}{2}}.$$

Or, if we please,

$$\text{Attraction } A + B = 2\pi\alpha - \frac{4}{3}\sqrt{2}\,\pi\alpha^{\frac{3}{2}},$$

$$,, \quad C = 2\pi\alpha - \frac{4}{3}\sqrt{2}\,\pi\alpha^{\frac{3}{2}},$$

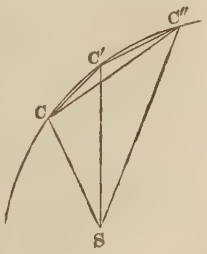
$$,, \quad D + E + F = \frac{8}{3}\sqrt{2}\,\pi\alpha^{\frac{3}{2}};$$

so that ultimately the attraction of the portion D + E + F vanishes in comparison with those of the portions A + B and C; and the attraction of these last, that is, of the whole shell, is $= 4\pi\alpha$, twice the attraction of an infinite plate of the thickness α.

Note on the Problem of the Determination of a Planet's Orbit from three Observations. By Prof. Cayley.

The principle of the solution given in the *Theoria Motus* may be explained very simply as follows:—

Consider three successive positions of C, C′, C″, of a planet revolving about the focus S; let n, n', n'', denote the doubles of the triangular areas C′SC″, CSC″, and CSC′ respectively (viz. the triangular area means the area of the triangle included between the two radius vectors and the chord joining their extremities), r' the radius vector SC′; θ'', θ, the times of describing the arcs CC′ and C′C″ respectively, the units of time and distance being such that the time



is equal to the double area divided by the square root of the half latus rectum ($t = 2\pi a^{\frac{3}{2}}$ for the Period in a circular or elliptic orbit).

Then writing,

$$P = \frac{n''}{n}, \quad Q = 2\left(\frac{n+n''}{n'} - 1\right) r'^3,$$

(observe that $n + n'' - n'$ is = twice the triangle C C′ C″), for neighbouring positions of the planet, the values of P and Q are *approximately* $= \frac{\theta''}{\theta}$ and $\theta\,\theta''$ respectively: the solution consists in the determination of an orbit for which P and Q have these approximate values; then, by means of such approximate orbit, the values of P and Q are more accurately determined, and by means of these new values of P and Q, a new determination is effected of the orbit: and so on, to the requisite accuracy of approximation.

The foregoing approximate values of P and Q respectively are deduced from the accurate values

$$P = \frac{\theta''\eta}{\theta\eta''}, \quad Q = \frac{\theta\,\theta''}{\eta\eta''}\frac{r'^2}{r'r''}\frac{1}{\cos f\,\cos f''\,\cos f'''};$$

where r, r', r'', are the radius vectors S C, S C′, S C″; $2f, 2f', 2f''$, are the angular distances C′ S C″, C S C″, C S C′ ($f = f' + f''$) and η, η', η'', are the ratios of the sectorial areas C′ S C″, C S C″, C′ S C″, to the triangular areas represented by the same letters respectively: the doubles of the sectorial areas are thus $n\eta, n'\eta'$, and $n''\eta''$, and if the half latus rectum be denoted by p, then we have

$$\sqrt{p} = \frac{n\eta}{\theta} = \frac{n'\eta'}{\theta'} = \frac{n''\eta''}{\theta''};$$

and it thus at once appears that the accurate value of P is $= \frac{\theta''\eta}{\theta\eta''}$, as above. To obtain the expression for Q, taking $\varphi, \varphi', \varphi''$, for the true anomalies (and, for greater symmetry, writing for the moment $\nu, -\nu', \nu'', g, -g', g''$, in place of n, n', n'', f, f', f'', respectively), we have

$$r = \frac{p}{1 + e\cos\varphi}, \quad 2g = \varphi'' - \varphi',$$

$$r' = \frac{p}{1 + e\cos\varphi'}, \quad 2g' = \varphi - \varphi'',$$

$$r'' = \frac{p}{1 + e\cos\varphi''}, \quad 2g'' = \varphi' - \varphi,$$

$$(g + g' + g'' = 0);$$

whence identically,

$$\frac{\sin 2g}{r} + \frac{\sin 2g'}{r'} + \frac{\sin 2g''}{r''} = -\frac{4\sin g\sin g'\sin g''}{p},$$

or writing

$$\nu = r'r'' \sin 2g, \quad \nu' = r''r \sin 2g', \quad \nu'' = rr' \sin 2g''$$

this is

$$\nu + \nu' + \nu'' = -\frac{4rr'r'' \sin g \sin g' \sin g''}{p},$$

$$= -\frac{(rr'r'')^2 \sin 2g \sin 2g' \sin 2g''}{2prr'r'' \cos g \cos g' \cos g''}$$

$$= -\frac{\nu\nu'\nu''}{2prr'r'' \cos g \cos g' \cos g''}.$$

This is, in fact,

$$n - n' + n'' = \frac{nn'n''}{2prr'r'' \cos f \cos f' \cos f''},$$

or since

$$\frac{nn''}{p} = \frac{\theta\theta''}{n\eta''},$$

it is

$$2\left(\frac{n+n''}{n'} - 1\right) = \frac{\theta\theta''}{n\eta''rr'r'' \cos f \cos f' \cos f''},$$

viz. multiplying by r'^3, it is

$$Q = \frac{\theta\theta''}{n\eta''} \frac{r'^2}{rr''} \frac{1}{\cos f \cos f' \cos f''},$$

the above-mentioned value of Q.

On Personality in Observing Transits of the Limbs of the Moon. By E. Dunkin, Esq.

One result of the extreme accuracy of modern astronomical observations and reductions has been the detection of numerous well-marked personal differences, or discordances, which had hitherto escaped notice. One of the first discovered of these personalities, and perhaps the most constant of them all, is that existing in the determination of the absolute time of a transit of a star across the wires in the field of view of the telescope, an operation in which observers have been known to differ more than 1^s. Well-defined personal differences were also noticed by the late Mr. Sheepshanks in the micrometrical comparison-observations of the standard yard; similar personalities have been detected in the microscope-readings of the cross-wire bisection of a division of a graduated circle; and, lately, Mr. Stone has

pointed out a very small, but real, personal difference in the separate determinations of the error of collimation of the telescope of the Greenwich Transit-Circle, although the amount is too insignificant to affect the observations.

The personality of which the present paper treats, is one which for many years has been known to exist. For some time, however, its effect upon the observations was not investigated. So long ago as 1848, the year after the erection of the Altazimuth at the Royal Observatory, a very large personal discordance was found between the tabular errors in R.A. of the Moon observed with the Altazimuth by Mr. H. Breen and myself, by whom all the observations had, up to that time, been made with that instrument. As this comparison of results, deduced from the observations of different limbs of the Moon, is the first that I am aware of, in which the amount of personal discordance in observing the Moon's limbs, has been clearly and systematically ascertained ; I cannot do better than quote in this place the statement of the Astronomer Royal on this (as it appeared to him) singular instance of observational personality.

"The circumstances under which the large errors given by Mr. Breen occurred were so various, in respect of Moon's age and Moon's position in her orbit ; and the intermixture of the observations had been so complete, that there was no doubt whatever that this was the result of a difference in the mode of observation. And this was not a result of personal equation usually so called : for it was known from the investigations of personal equation, as exhibited in the clock-errors given by stars (which are confirmed by similar investigations made to the end of 1849), that the personal equation was small. Neither was it a different estimation of the Moon's diameter, for the difference of errors of Moon's R.A. is nearly the same, and in the same direction, whether the first limb or the second limb be observed. It is strictly speaking, a *difference between the personal equation for the Moon and that for the stars:* or it may be thus stated, that *the duration of the impression on the nerves of the eye, or the time occupied in bringing into comparison the impressions on the eye and on the ear, is not the same when the Moon is observed with the eye as when a star is observed with the eye.* The evidence of this is given by the following numbers.

"By observations of the first limb of the Moon from 1847, May 16, to 1848, May 28 :

	s
The mean of 45 errors of Moon's Tabular R.A., by Mr. Dunkin is ..	+0·53
The mean of 35 errors by Mr. Breen is	+0·99
Excess of Mr. Breen's	+0·46

"By observations of the second limb of the Moon through the same period :

	s
The mean of 33 errors, by Mr. Dunkin, is	+0·50
The mean of 27 errors, by Mr. Breen, is..	+0·80
Excess of Mr. Breen's	+0·30

(*Introduction, Greenwich Observations,* 1848, p. lxx.)

A similar comparison of the errors of the Moon's tabular N.P.D. gave no sensible difference between the two observers. The Astronomer Royal fully explains the probable reason of this in the same *Introduction.*

A subsequent comparison of my observations of the Moon made on the meridian with the Transit-instrument with those of the oldest and most experienced transit-observer then at Greenwich, resulted in showing that no difference existed between us; consequently the discordance was thrown entirely on Mr. Breen's observations, to which a considerable correction was afterwards applied.

As the use of Hansen's Lunar Tables was first adopted in the *Nautical Almanac* for 1862, I had intended to make the present investigation include the seven years from 1862 to 1868. Owing, however, to a protracted illness of one of the assistants in 1862 ; to the absence of two assistants in the island of Valencia for a considerable period ; and to the employment of another in more than the usual amount of equatorial work, the four principal observers were very unequally distributed during that year. For this reason, I have used the observations only for the six years from 1863 to 1868.

The method I have employed to exhibit the personal differences is a very simple one. My object has been to determine the yearly means, for each of the four observers, of all the tabular errors in R.A. of the Moon observed with the Transit-Circle and Altazimuth, separating those of each instrument into two groups, one consisting of all the observations before opposition, and the other containing all after opposition. As the intermixture of the observations is satisfactory in each year, particularly with the Altazimuth, the mean tabular errors become comparable for the four observers, Mr. Dunkin (D), Mr. Ellis (E), Mr. Criswick (C), and Mr. Carpenter (J. C.). To satisfy myself on this point I have, for the year 1868, arranged all the errors for each observer according to the Moon's age; the mean age agrees sensibly for all, both before and after opposition. Several observations with both instruments made by other observers I have omitted solely on the ground of the paucity of the individual numbers.

The following Tables need no special explanation, further than to state that they are only intended to give the numerical results from the data as published in the columns of the *Greenwich Observations.* That the personal discordances which they exhibit are real, there can be no manner of doubt; they are probably due principally to the different values of diameter given by each observer's observations, but how much of the personality is pre-

cisely belonging to the observations of the first or second limb is a question very difficult to answer.

The transits of the first and second limbs of the Moon, and with the Altazimuth also of the upper and lower limbs, over the systems of wires in the field of view of the telescope, are sensibly very different operations, the Moon in its progress in one case approaching the wire, and in the second case receding from it. Every person who has had any experience in this kind of observation cannot have failed to notice that the attention of the observer's mind is here occupied in performing two different modes of estimation. From this cause it is probable that the observations of the two limbs are not made under analogous circumstances, and therefore a different amount of personality may exist in each, or, in other words, the methods of two observers may be similar in the observation of one limb, and be more or less discordant in another.

Altazimuth.

In the formation of Table I. I have extracted all the tabular errors in R.A. of the Moon as determined from the Altazimuth observations. These errors have been classified into groups according to the initial of the observer's name, keeping distinct those resulting from observations made before and after opposition. The yearly means of each group are contained in the Table.

Table I.

Before Opposition.

Year.	Mean Error of Tabular R.A. by D.	E.	C.	J.C.
	s	s	s	s
1863	−0·156	−0·296	−0·139	−0·388
1864	−0·030	−0·173	−0·093	−0·363
1865	+0·005	−0·155	+0·007	−0·253
1866	+0·071	−0·059	0·000	−0·162
1867	+0·213	−0·014	+0·106	−0·057
1868	+0·162	+0·059	+0·104	−0·035
Mean	+0·044	−0·106	−0·003	−0·210

After Opposition.

Year.	Mean Error of Tabular R.A. by D.	E.	C.	J.C.
	s	s	s	s
1863	−0·073	−0·136	−0·125	−0·122
1864	+0·016	+0·014	+0·016	−0·076
1865	+0·110	+0·014	+0·041	+0·064
1866	+0·078	+0·044	+0·041	+0·119
1867	+0·241	+0·047	+0·236	+0·184
1868	+0·242	+0·176	+0·235	+0·256
Mean	+0·102	+0·027	+0·074	+0·071

By taking the differences between the corresponding mean yearly tabular errors the numbers in the following Table are obtained. If no personality existed, and if the adopted semi-diameter used in the reductions were strictly correct, these differences would have been insensible for each observer, provided the mean tabular errors are assumed to be constant throughout the mean lunation.

Table II.

Difference between the Mean Tabular Errors of R.A. of Moon determined before and after Opposition.

Year.	D.	E.	C.	J.C.
	s	s	s	s
1863	+0·083	+0·160	+0·014	+0·266
1864	+0·046	+0·187	+0·109	+0·287
1865	+0·105	+0·169	+0·034	+0·317
1866	+0·007	+0·103	+0·041	+0·281
1867	+0·028	+0·061	+0·130	+0·241
1868	+0·080	+0·117	+0·131	+0·291
Mean	+0·058	+0·133	+0·077	+0·281

A cursory examination of the numbers in the two preceding tables will show plainly that a well-marked regular personality of some kind is exhibited in each year, without exception. It appears that all the observers give a sensibly smaller error of tabular Right Ascension in the first half of the lunation than in the second half, varying from $+0^{s}\cdot06$ from my observations to $+0^{s}\cdot28$ from those of Mr. Carpenter, the difference being equivalent to $+0^{s}\cdot22$, or $3''\cdot30$. If we were to assume that these personal differences arise chiefly from different methods of estimation of the true position of the Moon's limb, and that a similar amount of personality exists in the observations of the upper and lower limbs ; then the numbers in the above table would seem to show that the semi-diameter of the Moon determined by Mr. Carpenter differs from that resulting from my observations by about $1''\cdot50$. The following table is computed from Table I., by adopting for convenience the usual habit of referring all the observer's means to mine, which for the purpose we will suppose to equal 0. The numbers in the table are therefore differences of my mean results for each year from those of the other observers

Table III.

Before Opposition.

Year.	D—E.	D—C.	D—J.C.
	s	s	s
1863	+0·140	−0·017	+0·232
1864	+0·143	+0·063	+0·333
1865	+0·160	−0·002	+0·258
1866	+0·130	+0·071	+0·233
1867	+0·227	+0·107	+0·270
1868	+0·103	+0·058	+0·197
Mean	+0·151	+0·047	+0·254

After Opposition.

Year.	D—E.	D—C.	D—J. C.
	s	s	s
1863	+0·063	+0·052	+0·049
1864	+0·002	0·000	+0·092
1865	+0·096	+0·069	+0·046
1866	+0·034	+0·037	−0·041
1867	+0·194	+0·005	+0·057
1868	+0·066	+0·007	−0·014
Mean	+0·076	+0·028	+0·032

Referring to the preceding table it may be remarked that, before opposition, Mr. Ellis's mean tabular error agrees nearly with the mean of all, or is only slightly in excess of it. Mr. Criswick's mean tabular error is between the mean and mine, those of Mr. Carpenter and myself being at opposite ends of the scale. After opposition, excepting the comparison with Mr. Ellis in 1867, which from some unknown accidental cause stands out from the other years, no difference in the table reaches $0^{s}\cdot1$, while in the majority of instances the amount is below $0^{s}\cdot05$.

Although the major part, if not all, of the personality apparently belongs to the first half of the lunation when the first limb of the Moon has been observed, yet it is very probable, and even certain, that some part of it arises from other causes than by the different methods of observing the transits of the first and second limbs. In the reductions of the Altazimuth observations, the error of tabular R.A. is not directly obtained from the simple transit of the limb, as with the Transit-circle, but from a comparatively complicated series of observations and calculations, depending not only upon transits of the right or left limb, but also on transits of the upper or lower limb, in which personality creeps in as much probably as in the azimuthal transits, and upon micrometrical readings of the divisions of the horizontal and vertical circles. In vertical transits, the limb observed depends upon the position of the Moon with respect to the meridian and horizon at the time of observation. I find from a special examination of the printed volumes of the *Greenwich Observations*, that in the first half of the lunation the observations of the upper and lower limbs are nearly equally divided, and that nearly all those of the lower limb are observed west of the meridian, and nearly all those of the upper limb east of the meridian. The result of this is that, if the adopted semi-diameter employed in the reductions is too small or too great, the transits of the upper and lower limbs made on opposite sides of the meridian, would affect the resulting tabular error of R.A. in the same direction, and would tend to produce, if the semi-diameter be too great, a large negative error in the first half of the lunation. After opposition, the lower limb of the Moon, with very few exceptions, is always observed east of the meridian. These complications make the discovery of the true cause of the personality being

apparently more in one limb than another a very difficult matter indeed.

If we may suppose that the principal cause is a question of personal semi-diameter, then it would not be difficult to apply separate corrections to make the errors for both azimuthal limbs alike, but there would still be an outstanding personality left in the mean absolute tabular errors. The quantities to apply both to the azimuthal and vertical semi-diameters now in use would be small ; for each observer they would be,—

	s		″
D =	− 0·02	or	− 0·30
E =	− 0·05		− 0·75
C =	− 0·03		− 0·45
J. C =	− 0·10		− 1·50

Before opposition, as the observations of the upper and lower limbs are about equal, the lower limb being observed after the meridian, and the upper limb before the meridian, a decrease in the adopted vertical and horizontal semi-diameters tends to increase *positively* the error of tabular R.A. After opposition, a decrease in the semi-diameter in azimuth tends to diminish the tabular error of R.A., and the lower limb being nearly always observed east of the meridian, the tabular error is also diminished if the semi-diameter be decreased. I have computed, for mean hour-angle and N.P.D. the exact correction due to a decrease of 1″·50 in both the azimuthal and vertical semi-diameters.

	s
The compound effect on tabular error in R.A before opposition is	+ 0·14
That due to correction of azimuthal semi-diameter	+ 0·09
„ „ vertical „	+ 0·05

After opposition the same numbers will apply with changed signs. If we apply these corrections to the means of Mr. Carpenter's observations in Table I., and proportional corrections to the means of the other observers, the individual mean tabular errors before and after opposition become accordant, although there still remains a visible personality in the absolute errors determined by the different observers.

Before Opposition.	After Opposition.
	s
D = + 0·07	+ 0·07
E = − 0·04	− 0·04
C = + 0·04	+ 0·07
J.C = − 0·07	− 0·07

Transit Circle.

I have treated the tabular errors in R.A. deduced from the Transit-circle observations in a similar manner to the Altazimuth

observations. The materials are, however, much fewer, and the intermixture of the observers is not quite so complete. This comparative inferiority, with respect to numbers, is however, in a great measure compensated by the more direct manner with which the tabular errors have been obtained, and by the superior optical power of the instrument. The yearly means of the tabular errors are contained in the next table.

Table IV.

Before Opposition.

Year.	Mean Error of Tabular R.A. by D.	E.	C.	J.C.
	s	s	s	s
1863	−0·204	−0·249	−0·168	−0·249
1864	−0·071	−0·129	−0·025	−0·187
1865	+0·055	−0·072	+0·056	−0·089
1866	+0·131	+0·036	+0·147	+0·027
1867	+0·186	+0·108	+0·245	+0·101
1868	+0·220	+0·158	+0·270	+0·130
Mean	+0·053	−0·025	+0·087	−0·044

After Opposition.

Year.	Mean Error of Tabular R.A. by D.	E.	C.	J.C.
	s	s	s	s
1863	−0·111	−0·060	−0·04	−0·118
1864	+0·096	+0·060	+0·131	+0·060
1865	+0·174	+0·102	+0·168	+0·217
1866	+0·254	+0·136	+0·277	+0·194
1867	+0·350	+0·332	+0·362	+0·375
1868	+0·400	+0·323	+0·423	+0·399
Mean	+0·194	+0·149	+0·226	+0·188

The differences of each observer's mean results from the mean of all are

Before Opposition.	After Opposition.
s	s
D = +0·035	+0·004
E = −0·043	−0·041
C = +0·069	+0·036
J. C = −0·062	+0·002

By taking the difference between the corresponding tabular errors determined before and after opposition in each year, Table IV., it will be seen that in every instance the errors are in excess after full Moon. The mean quantity in excess for the six years is as follows:—

$$D = +0^{s}\cdot141$$
$$E = +0\cdot174$$
$$C = +0\cdot139$$
$$J.\,C. = +0\cdot232$$

The mean of all is $+ 0^{s}\cdot171$, showing that the tabular duration of the Moon's passage over the meridian deduced from Hansen's Tables should be diminished by about $0^{s}\cdot085$. This quantity agrees in the same direction with that deduced directly from transits of the first and second limbs at opposition, as given in the Introduction to the *Greenwich Observations* from 1863 to 1868. The next table corresponds with Table III.

Table V.

Before Opposition.

Year.	D.—E.	D.—C.	D.—J.C.
	s	s	s
1863	+0·045	−0·036	+0·045
1864	+0·058	−0·046	+0·116
1865	+0·127	−0·001	+0·144
1866	+0·095	−0·016	+0·104
1867	+0·078	−0·059	+0·085
1868	+0·062	−0·050	+0·090
Mean	+0·078	−0·035	+0·097

After Opposition.

Year.	D.—E.	D.—C.	D.—J.C.
	s	s	s
1863	−0·051	−0·107	+0·007
1864	+0·036	−0·035	+0·036
1865	+0·072	+0·006	−0·043
1866	+0·118	−0·023	+0·060
1867	+0·018	−0·012	−0·025
1868	+0·077	−0·023	+0·001
Mean	+0·045	−0·032	+0·006

With respect to the personal differences between the observers these numbers are confirmatory of those in the corresponding table in the Altazimuth results, but they are of a much smaller magnitude, as might be expected. If we place the means of the two sets side by side, their relative agreement for personality will be better seen.

	Before Opposition.		After Opposition.	
	Altazimuth.	Transit-Circle.	Altazimuth.	Transit-Circle.
	s	s	s	s
D − E	+0·151	+0·078	+0·076	+0·045
D − C	+0·047	−0·035	+0·028	−0·032
D − J.C.	+0·254	+0·097	+0·032	+0·006

The effect of the personality, whatever may be its cause, is visible in the tabular errors more in the first half of the lunation than in the second, as with the Altazimuth observations, but the differences of each observer's means from the mean of all are too small to trace the personality to any particular source. The result of this investigation has convinced me more than ever of the advantage derived from an intermixture of the observers when absolute places are required. It is now a well-known general fact that a long series of observations made by one observer may appear in most beautiful accord, and yet the mean of all may be considerably in error from the effect of some peculiar personality. On the other hand, where several observers are well intermixed, as in the observations now under discussion, personalities may exist, but their effect is most probably nearly eliminated when the results are grouped together. In personal habits of observation, it is not always easy to determine which observer approaches nearest to absolute truth, and it is very possible, and probable also, that that perfection is never attained even by the oldest or most experienced. Where several trained observers are employed in the same series of observations, it is very likely that the mean of all the observations is the correct result, and that it is nearly free from error arising from personality. But the importance of detecting the small personal differences which are found in almost all astronomical observations, and when discovered, to determine their amount, cannot be overrated. This importance must be my excuse for offering these few remarks to the Society, on a subject which is necessarily somewhat of a technical character, and which is probably interesting in a great measure to the professional astronomer alone.

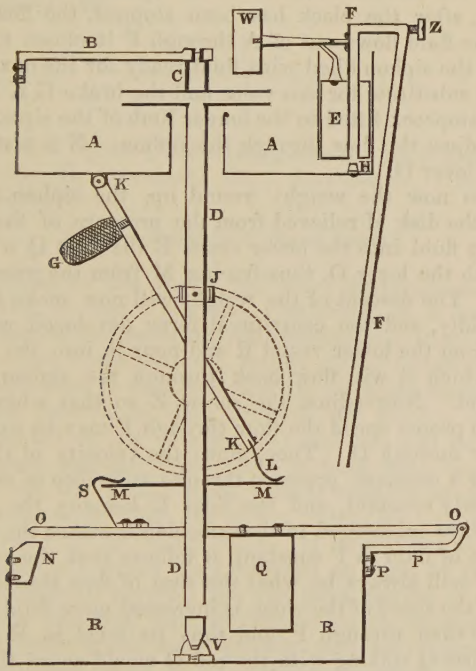
Greenwich, 1869, *April* 7.

Description of an Improved Driving-Clock.

By Sidney B. Kincaid, Esq.

The failure of the ordinary governor-clock to give in several instances which came under my own notice, when applied to Equatoreals of large size, a motion sufficiently regular for the purposes of ordinary astronomical observations, and its lack of the accuracy necessary for the successful conjunctive use of the spectroscope led me some considerable time since to give my attention to the devising of a mechanical system which should form an apparatus more nearly perfect in its performance. Many and great interruptions have hindered the task and compelled me to delay the completion of my plan. I lay it before the members of the Royal Astronomical Society now, before having it put to the test of actual experiment, because the principle in which it differs from other driving-clocks and the mode of using it can be seen, and their advantages in practice judged of and determined by

mere inspection of the scheme, and because there may be among the members of the Society some having the opportunity and the disposition to do that which I could not myself undertake for some years.



R is the lower vessel having in the centre at the bottom a conical bearing V for the lower end of the tubular shaft D to rotate in. The upper vessel A is supported over the lower one by columns, not shown in the figure, and carries at the end of an arm B fixed to its side a cylindrical bearing for the upper pivot of D. This shaft D is furnished with apertures into its interior below the surface of the fluid in the lower vessel, and with a pair of of parallel horizontal plates attached near its upper end, so that the space between these plates communicates with the bore of the shaft; the train of wheels, by which the driving-power of the weight is transferred in the usual way to the tangent-screw of the telescope, is connected with the shaft through the vertical crown-wheel, shown by the dotted outline in the diagram, which works with the pinion J on the shaft D. To the shaft is also fixed the plane horizontal concentric disk M, on which rests the steel spring S attached to the lever O which carries the float Q, and plays on a hinge at the end of the arm P fixed to the side of the lower vessel R. To the under side of A is attached by a hinge the rod

K, so that by moving the handle G upwards the spring L at the end of the rod can be made to press upon the disk M with sufficient force to stop the clock. Between guides in the upper vessel works a float E which carries the siphon F, counterposed by the weight W. H is the conical value to fit the opening of the siphon, so that when, after the clock has been stopped, the float E settles down as the fluid flows out of A through F it closes the opening and leaves the siphon filled with fluid ready for the next start. A convenient substitute for this value and the brake G K L might be found in a stopcock fitted to the longer limb of the siphon. Z is a screw to adjust the flow through the siphon. N is a stop for the end of the lever O.

Suppose now the weight wound up, the siphon filled with fluid, and the disk M relieved from the pressure of the spring L. By pouring fluid into the lower vessel R the float Q will be made to rise with the lever O, thus freeing M from the pressure of the spring S. The descent of the weight will now make the shaft D rotate rapidly, and the centrifugal force developed will exhaust the fluid from the lower vessel R and pour it into the upper one A, from which it will flow back through the siphon F into the lower vessel. Now adjust the screw Z so that when the clock goes at the proper speed the flow through F may be exactly equal to the flow through D. Then, since the velocity of the flow of a fluid under a constant pressure through an orifice of constant size is very nearly constant, and the float E keeping the end of the siphon always submerged to the same depth makes the pressure of the column of fluid in F constant, it follows that the rate of flow through F will always be what the rate of flow through D ought to be. If the speed of the clock is increased more fluid would flow through D than through F, and thus its level in R would fall, and the float Q sinking with the rod O would cause the spring S to press more hardly on M and to thus diminish the speed; if, on the other hand, an increase of resistance in any part retarded the clock, more fluid would flow through F than through D, and the float Q would rise and diminish the pressure of the spring S on M and increase the speed.

It is to be noticed that in this apparatus the regulator is perfectly independent of the driving-train, that every variation from the normal rate is instantly and *absolutely compensated*, and that in constructing such a clock the sensitiveness can be increased by increasing the normal flow.

The greater mobility of a fluid than of any system of mechanical joints made me determine, in the first instance, that it was by the use of a fluid that I should endeavour to attain the object I had in view. Whatever prejudice there may be against the use of a fluid for the purpose is unreasonable, forasmuch as the only objection which could be rationally urged—its liability to freeze, and thus to render the apparatus useless for the time being during the colder seasons of the year—does not apply either to the fluid I myself intended to be used—castor-oil, selected from specimens

which are not congealable by the winter cold of this country, and such are readily to be met with in commerce—or to that which Mr. Huggins suggested to me, glycerine.

Trinity College, Cambridge,
27 March, 1869.

On the Practical Speed of Electricity through 7200 Miles of Land Wire. By G. Davidson, Esq., Astronomer, United States Coast Survey.

(*Communicated by C. Piazzi Smyth.*)

Observations to the above end were made on the night of Feb. 28, 1869, at San Francisco, California, on a loop of wire extending thence to Cambridge, Massachussets, 3600 miles, and returning to San Francisco; the return signal being received on a chronograph close to that receiving the outgoing signal.

The total interval of time between the records of the two signals amounted to 0·8 of a second; and this may be called the *practical* speed through 7200 miles of land wire, because that length was cut up in the usual service manner of telegraphic companies, into 11 portions, say of 650 miles each; these portions being connected by as many relays (or repeaters, as they are called in America), which send forward on each of these occasions a new electrical current into the next section of the wire; and such new electrical current would travel through its new 650 miles of wire much more quickly than an original current through the second, third, or any subsequent continued length of 650 miles.

The integral result may, therefore, be looked on as a safe and satisfactory multiplication by 11 of the time occupied by electricity in both travelling through 650 miles of land wire, and working *one* relay of the kind, whatever that may be, now used in the United States; the time occupied in effecting such work being apparently 0·07 of a second.

Instrument for Sale.

A very fine Equatoreal, by Cooke of York; almost new. Aperture 6 inches, clock movement, eye-pieces, finder, &c. Also, a Sidereal Clock and Chronometer, by Dent. Address Miss Naylor, Altrincham.

CONTENTS.

	Page
Fellow elected	229
On the Rotation of *Mars*, by Mr. Proctor	ib.
On the Meteoric Shower of November 1868, as seen at the Royal Observatory, Cape of Good Hope, by Mr. Maclear	233
On some points connected with the Rediscussion of the Observations of the Transit of *Venus*, 1769, by Mr. Stone	236
Addition to Mr. Proctor's Paper on the Transit of *Venus* in 1874 ..	249
On some Effects of the Comparative Clinging of the Limb of *Venus* to that of the Sun in the Transit of 1874, as compared with that of 1882, by Mr. Stone	250
Occultations of Stars by the Moon and Phenomena of *Jupiter's* Satellites, observed at the Royal Observatory, Greenwich	252
Note on the Attraction of Ellipsoids, by Prof. Cayley	254
Note on the Problem of the Determination of a Planet's Orbit from three Observations, by Prof. Cayley	257
On Personality in Observing Transits of the Limbs of the Moon, by Mr. Dunkin	259
Description of an Improved Driving-Clock, by Mr. Kincaid	268
On the Practical Speed of Electricity through 7200 Miles of Land Wire, by Mr. Davidson	271
Instrument for Sale	ib.

Printed by STRANGEWAYS AND WALDEN, Castle Street, Leicester Square, and Published at the Apartments of the Society, May 3, 1869.